Assessing Economic Liberalization Episodes: A Synthetic Control Approach*

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Abstract

We use a transparent statistical methodology for data-driven case studies—the synthetic control method—to investigate the impact of economic liberalization on real GDP per capita in a worldwide sample of countries. Economic liberalization is measured by a widely used indicator that captures the scope of the market in the economy. The methodology compares the post-liberalization GDP trajectory of treated economies with the trajectory of a combination of similar but untreated economies. We find that liberalizing the economy had a positive effect in most regions, but more recent liberalizations, in the 1990s and mainly in Africa, had no significant impact.

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1 Introduction

Theoretical results in international economics and growth theory largely point to a positive relationship between economic liberalization and economic welfare, but confirming this prediction empirically has proven to be a Sisyphean job. A major complication in identifying this relationship lies in the well-known limitations of cross-country econometric studies due to endogeneity or measurement issues (e.g., see Rodriguez and Rodrik, 2001). But also the alternative suggested by some authors (e.g., see Bhagwati and Srinivasan, 2001), namely country-specific case studies, faces limitations as these studies usually lack statistical rigor and are exposed to discretionary case selection.

In this article, we expand on Bhagwati and Srinivasan's (2001) suggestion and offer a set of empirical country studies on the effect of economic liberalization on the pattern of income per capita. At the same time, we provide a unified statistical framework to compare the income performance of open versus closed economies to minimize the impact of discretion in the analysis—or at least to make it transparent. In particular, we apply a recent econometric technique, the synthetic control method, to perform data-driven comparative case studies, which we view as a "third way" between standard cross-country estimators that are prone to suffer from multiple endogeneity issues and the hardly generalizable analysis of individual country episodes. We cover as large a sample as possible within the data constraints imposed by the proposed framework.

We use a worldwide panel of economies over the period 1963–2005 and evaluate the effect of a binary indicator of economic liberalization—derived by Sachs and Warner (1995), extended, updated, and revised by Wacziarg and Welch (2003, 2008)—on the outcome, namely the post-treatment trajectory of real GDP per capita. This binary indicator has been widely used in cross-country studies and therefore allows us to anchor our findings in the existing literature on the nexus between openness and income. Following Giavazzi and Tabellini (2005, p. 1298), we interpret it as a broad measure of "comprehensive reforms that extend the scope of the market, and in particular of international markets."

Within the synthetic control framework, we ask whether liberalizing the economy in year T leads to higher growth performance in the years $T+i$ (with $i \in [1, 10]$) compared to similar countries that have not opened up. The advantage of this approach lies in the transparent estimation of the counterfactual outcome of the treated country, namely a linear combination of untreated countries—the synthetic control. The comparison economies that form the synthetic control unit are selected by an algorithm based on their similarity to the treated economy before the treatment, both with respect to relevant covariates and past realizations of the outcome variable (real GPD per capita).

We study all episodes of economic liberalization that took place in the world since 1963 as long as they qualify for our empirical framework. In the sample selection procedure, we require that for each country that liberalized its economy in a certain year, there should be a sufficient number of comparison countries in the same region that did not liberalize before or soon thereafter. This feature distinguishes our study from the standard cross-sectional work in an important way: we pay particular attention to the question whether there is enough variation in the treatment within a given geographical region (that is, whether treated and comparison units share a "common support"). This transparent sample selection procedure indicates that for some regions we are skating on very thin ice when we try to identify the effect of economic liberalization on income, because reform waves reduce the number of available comparison countries after a given year. Indeed, as shown in Billmeier and Nannicini (2009), the failure of standard cross-sectional estimators to control for the existence of such a common support leads to quite far-fetched country comparisons underlying standard estimation results. Forcing us to select a more appropriate set of comparison units is thus a first advantage of the synthetic control method. Moreover, the transparent construction of the estimated counterfactual safeguards against the risk of drawing inference from (hidden) parametric extrapolation.

A second advantage of the proposed statistical framework is that, unlike most of the estimators used in the literature, it can deal with endogeneity from omitted variable bias by accounting for the presence of *time-varying* unobservable confounders. This feature improves upon panel models such as fixed effects or difference-in-differences, which can only account for *time-invariant* unobservable confounders. A remaining limitation, however, is that economic reforms might be triggered by the anticipation of future growth prospects, thus leading to endogeneity from reverse causation. As long as growth expectations are not captured by the unobservable heterogeneity included in the model, this would bias the findings of the synthetic control approach.

As an additional caveat, we also note the difficulty of comparing economic reforms across countries because liberalization efforts might take very different forms. Moreover, economic liberalization cannot be decoupled from the political background (e.g., see the diverse economic reform agendas that came with with political transformation in the Comecon countries). When interpreting the empirical results on each economic liberalization episode, we therefore try to account for the fact that our treatment may encompass heterogeneous reforms at the country level.

In selecting the potential comparison countries, we further exploit the flexibility of the method and, for each treated country, we implement two types of experiments. In type-A experiments, we restrict the control group to eligible countries in the same macro region of the treated (Asia, Latin America, Africa, and Middle East); in type-B experiments, we incorporate all eligible countries (available across all regions) into the control group. As a result, type-A experiments ensure the existence of a common support between treated and comparison countries according to factors related to geographical location and exclude the rather far-fetched country comparisons discussed by Billmeier and Nannicini (2009), while type-B experiments increase sample size and the power of our test.

Our empirical findings show that, for many countries that we can analyze, economic liberalization had a positive effect on income per capita. However, we find a lot of heterogeneity in the results across regions and time. In particular, countries that liberalized their economy after 1990—many of which are located in Africa—did not benefit from these reforms in terms of higher GDP compared to similar, but closed economies.

The remainder of this paper is structured as follows. In Section 2, we briefly review the relevant literature. In Section 3, we present the data sources and variables of interest. The synthetic control approach is discussed in Section 4. Section 5 empirically explores the effect of economic liberalization on income patterns. Section 6 concludes.

2 Related Literature

While a large body of broadly supportive theoretical literature exists, providing conclusive and robust empirical evidence on the intuitively positive effect of market liberalization on income has been a challenging endeavor and hotly debated topic, complicated by a number of factors. Regarding the connection between trade openness and growth, Dollar (1992) finds that countries with an inward-oriented trade regime—as reflected by relatively high price and protection levels, and real overvaluation of the currency—could increase their growth rates by 1.5–2 percentage points with a shift to more outward-oriented trade policies. Sachs and Warner (1995, p. 47) provide evidence that, in an augmented Barro-type growth regression, being "open" is correlated with growth convergence among countries, and that "open economies grow, on average, by 2.45 percentage points more than closed economies, with a highly statistically significant effect." Edwards (1992, 1998) finds that for eight different measures of openness, the impact on TFP growth is positive and significant in 13 out of 18 estimates, noting though that the effect of other covariates is often larger. Presenting historical evidence, Vamvakidis (2002) finds that a positive correlation between openness and growth can only be detected in the data starting from 1970.

Rodriguez and Rodrik (2001), on the other hand, cast doubt on the robustness of these affirmative results. They point out that much of the literature produced during the 1990s documenting a positive effect suffers from various weaknesses, related especially to the liberalization/openness measure and the econometric modeling approach, which they view as suffering from regressor endogeneity as it is often based on OLS estimates. More specifically, they argue that the commonly employed trade openness indicator, developed in Sachs and Warner (1995), is subject to a number of shortcomings—among the most notable ones that (i) the indicator captures much more than just openness to trade and should be interpreted accordingly, and that (ii) the positive correlations found in Dollar (1992), Sachs and Warner (1995), and Edwards (1998) are not robust.[1] DeJong and Ripoll (2006) follow up on one of the suggestions voiced in Rodriguez and Rodrik (2001) and develop an alternative measure of trade barriers—*ad valorem* tariff rates. In a sample of 60 countries, they find that the correlation between trade barriers and income is negative for rich countries but positive in poorer countries. Furthermore, Levine and Renelt (1992) and Temple (2000) apply extreme-bounds analysis to show that the results of cross-country regressions are not robust to even small changes in the conditioning information set.

Bhagwati and Srinivasan (2002) question cross-country evidence on the openness-growth nexus on a more fundamental level, and promote descriptive case studies as a way to avoid the pitfalls of standard cross-country evidence. They point out that "cross-country regressions are a poor way to approach this question" and that "the choice of period, of the sample, and of the proxies, will often imply many degrees of freedom where one might almost get what one wants if one only tries hard enough!" (p. 181). On similar grounds, Pritchett (2000) argues for detailed case studies of individual countries.

This support for comparative case studies can be interpreted as one attempt to sidestep

[1]More recent contributions in the political economy literature—such as Giavazzi and Tabellini (2005) and Persson and Tabellini (2006), see further below—interpret the Sachs-Warner dummy more appropriately as a broader indicator of economic liberalization, no longer of trade openness alone. See Section 3 for a more detailed discussion on this point.

the weaknesses of standard cross-country regressions, but of course the adoption of estimators explicitly devised to overcome some of these limitations has been the main alternative solution. Broadly speaking, two main econometric strategies have been used in this respect: Instrumental Variables (IV) and panel methods.

First, to control for the endogeneity issues in the early literature, a number of contributions have turned to IV as a remedy. Using a gravity model, Frankel and Romer (1999) instrument for trade shares in GDP with geographic characteristics and show that the positive effect of trade on income is underestimated when using OLS estimators. Quantitatively, they find that a one-percent increase in the (instrumented) trade share in GDP raises income per capita by between two and three percent depending on the sample, doubling and tripling the respective OLS coefficient. In another application of a gravity model, Irwin and Terviö (2002) find a positive effect running from trade to growth by isolating geographical components of openness that are assumed independent of economic growth, including population, land area, borders, and distances. Their results confirm those of Frankel and Romer (1999)—that the 2SLS estimate significantly exceeds the OLS estimate—for the whole 20th century: in their results, by a factor of 2.6 on average. Another recent example for the IV approach is Romalis (2007), who instruments the openness measure for developing countries with tariff barriers by the United States. He finds that eliminating existing tariffs in the developed world would increase developing countries' annual GDP growth rates by 0.6 to 1.6 percentage points. All of these instruments, however, apply to quantitative trade volume measures but not to measures of the policy stance and are therefore less relevant in our context. Furthermore, geographical instruments might have direct effects on growth, thereby violating the exclusion restrictions.

Second, the possibility to combine the analysis of time series with cross-sectional information has spurred another strand in the trade-and-growth literature that employs panel methods to control for time-invariant unobservable country effects. An early example is

Harrison (1996), who uses fixed-effect estimators and finds a stronger impact of various openness indicators compared to standard cross-country regressions. Wacziarg and Welch (2003, 2008) further the discussion in three directions: they update, expand, and correct the economic liberalization indicator by Sachs and Warner (1995); they show that the Sachs and Warner (1995) results of a positive effect of trade on growth break down if extended to the 1990s in a cross-sectional setup; and they provide evidence in a panel context that, even in the 1990s, there is a positive effect of trade on growth when the analysis is limited to within-country effects. According to their results, countries on average grow faster by about 1.5 percentage points after liberalizing. Another typical panel approach—the difference-in-differences estimator—is used by Slaughter (2001) to infer the effect of four very specific liberalization events on income growth dispersion; he finds no systematic link between liberalization and per capita income convergence.

Giavazzi and Tabellini (2005) also apply a difference-in-differences approach to study the interactions between economic and political liberalizations. They find a positive and significant effect of economic liberalization on growth, but they note that this effect may not be entirely attributed to international trade, as liberalizations tend to be accompanied by other policy improvements. According to the evidence they present, economic liberalizations speed up growth by about one percentage point and raise the share of investment by almost two percentage points of GDP.

In this paper, we build a bridge between the case-study approach and the econometric response to the weaknesses of cross-country estimators by employing synthetic control methods: A recent methodology that builds data-driven comparative case studies within a unified statistical framework and accounts for time-varying unobservable confounding factors.

3 Data

To anchor our results in the existing literature, we draw on a dataset used recently by Giavazzi and Tabellini (2005) and Persson and Tabellini (2006). The data cover about 180 countries over the period 1963–2000. As a measure of economic liberalization, we use the binary indicator by Sachs and Warner (1995) as extended, updated, and revised by Wacziarg and Welch (2003, 2008); short SWWW. According to this indicator, a country is considered closed in any given year if at least one of the following conditions is satisfied: (i) average tariffs exceed 40 percent; (ii) non-tariff barriers cover more than 40 percent of its imports; (iii) it has a socialist economic system; (iv) the black market premium on the exchange rate exceeds 20 percent; and (v) much of its exports are controlled by a state monopoly. When applying the synthetic control method in a panel setup, in line with Giavazzi and Tabellini (2005, p. 1298), we refer to the *treatment* as the event of economic liberalization (i.e., "comprehensive reforms that extend the scope of the market, and in particular of international markets") after experiencing a closed economy in the preceding years according to the SWWW indicator. Our treatment thus intends to capture policy changes that reduce the constraints on market operations below a critical threshold along the above five dimensions. It is true that the SWWW indicator is mainly defined in terms of trade policies, but in many countries contained in our sample trade openness was accompanied by other market-oriented policy measures, and this indicator should therefore be interpreted more broadly (Rodriguez and Rodrik, 2001).

We are interested in estimating the effect of economic liberalization on an outcome variable reflecting economic wellbeing. Based on the theory, we expect economic liberalization to have a step effect that could build up over time given the lags of the economy; we do not expect divergent long-run growth paths between a liberalizing economy and its counterfactual that did not liberalize. Therefore, for the variable reflecting wellbeing, we use the time series of real GDP per capita (measured in 2002 US$), so as to focus on the dynamic impact of liberalization over time, not its one-off effects on the individual income level. The series comes from the IMF *World Economic Outlook* database, as this allows us to extend, in a consistent way, the post-treatment period for the outcome variable up to 2005 when necessary (i.e., when liberalizations take place toward the end of the original dataset).

Finally, from the original dataset, we draw a set of covariates used in the literature on cross-country growth regressions (e.g., see Barro, 1991); that is, the annual observations on: GDP per capita before the treatment; investment as a share of GDP; population growth; secondary school enrollment; the average inflation rate; and a democracy dummy. We use these variables as covariates only when they are available for at least one year in the pre-treatment period, which is not always the case for inflation and democracy.

4 Methodology: The Synthetic Control Approach

An estimation approach recently implemented for comparative case studies—the synthetic control method (SCM) developed by Abadie and Gardeazabal (2003) and extended in Abadie, Diamond, and Hainmueller (2010)—can be promisingly applied to the investigation of the impact of economic liberalization. Under this approach, a weighted combination of potential control countries—namely, the synthetic control—is constructed to approximate the most relevant characteristics of the country affected by the intervention. After the regime change (economic liberalization) takes place in a specific country, the SCM can be used to estimate the counterfactual situation of this country in the absence of the regime change by looking at the outcome trend of the synthetic control.

Formally, it is useful to reason in terms of potential outcomes in a panel setup. Assume that we observe a panel of $I_C + 1$ countries over T periods. Only country i receives the treatment (that is, liberalizes its economy) at time $T_0 < T$, while the remaining I_C

potential control countries remain closed. The treatment effect for country i at time t can be defined as:

$$\tau_{it} = Y_{it}(1) - Y_{it}(0) = Y_{it} - Y_{it}(0), \tag{1}$$

where $Y_{it}(1), Y_{it}(0)$ stand for the potential outcome with and without treatment, respectively. The estimand of interest is the vector $(\tau_{i,T_0+1}, ..., \tau_{i,T})$. For any period t, the estimation of the treatment effect is complicated by the missing counterfactual $Y_{it}(0)$.

Abadie, Diamond, and Hainmueller (2010) show how to identify the above treatment effects under the following general model for the potential outcomes of all units:

$$Y_{jt}(0) = \delta_t + \nu_{jt} \tag{2}$$

$$Y_{jt}(1) = \tau_{jt} + \delta_t + \nu_{jt}, \tag{3}$$

with $j = 1, \ldots, I_C + 1$. Given the treatment assignment mechanism described above, τ_{jt} is different from zero only when $j = i$ and $t > T_0$. Besides the (dynamic) treatment effects τ_{jt}, potential outcomes depend on a common factor δ_t and an error ν_{jt}. Assume that ν_{jt} can be expressed by the following factor model:

$$\nu_{jt} = Z_j\theta_t + \lambda_t\mu_j + \epsilon_{jt}, \tag{4}$$

where Z_j is a vector of relevant observed covariates that are not affected by the intervention and can be either time-invariant or time-varying; θ_t is a vector of time-specific parameters; μ_j is a country-specific unobservable; λ_t is an unknown common factor; and ϵ_{jt} are zero-mean transitory shocks. The j-subscript to the Z-vector does not impose any restriction on the covariates included in the model, which may actually vary with time or not, and may be pre- or post-treatment, as long as they do not depend on the intervention.

In our context, as all the elements in Z_j (GDP values in every pre-treatment year, average population growth, secondary school enrollment, investment share, inflation, and democracy) refer to the pre-liberalization period, the assumption that they are not affected by the treatment means that we have to rule out "anticipation" effects, i.e., that these variables change in response to the anticipation of the future reform, before the liberalization actually takes place. Interestingly, the above model allows for the impact of unobservable country heterogeneity to vary with time, while standard difference-in-differences or fixed-effect specifications impose λ_t to be constant over time.

Define $W = (w_1, ..., w_{I_C})'$ as a generic $(I_C \times 1)$ vector of weights such that $w_j \geq 0$ and $\sum w_j = 1$. Each possible choice of W corresponds to a potential synthetic control for country i. Further define $\bar{Y}_j^k = \sum_{s=1}^{T_0} k_s Y_{js}$ as a generic linear combination of pre-treatment outcomes. Abadie, Diamond, and Hainmueller (2010) show that, as long as we can choose w^* such that

$$\sum_{j=1}^{I_C} w_j^* \bar{Y}_j^k = \bar{Y}_i^k \qquad \text{and} \qquad \sum_{j=1}^{I_C} w_j^* Z_j = Z_i, \tag{5}$$

then

$$\hat{\tau}_{it} = Y_{it} - \sum_{j=1}^{I_C} w_j^* Y_{jt} \tag{6}$$

is an unbiased estimator of τ_{it}. Condition (5) can hold exactly only if $(\bar{Y}_i^k, Z_i)$ belongs to the convex hull of $[(\bar{Y}_1^k, Z_1), ..., (\bar{Y}_{I_C}^k, Z_{I_C})]$. Hence, in practice, the synthetic control weights w^* are estimated in a completely non-parametric fashion and are selected so that condition (5) holds approximately: the distance (or pseudo-distance) between the vector of pre-treatment characteristics of the treated country and the vector of the pre-treatment characteristics of the potential synthetic control is minimized with respect to w^* and according to a specified metric.[2] Specifically, let X_1 be the vector of pre-treatment charac-

[2]Note that this distance minimization problem resembles that of covariate matching estimators in the microeconometric treatment evaluation literature, where the Mahalanobis or normalized Euclidean distances are commonly used as metrics.

teristics for the treated country, and X_0 the matrix collecting the vectors of pre-treatment characteristics of the untreated countries. The vector w^* is then chosen to minimize the distance $||X_1 - X_0 w||_V = \sqrt{(X_1 - X_0 w)' V (X_1 - X_0 w)}$, where V is a $(k \times k)$ symmetric and positive semidefinite matrix. To assign larger weights to pre-treatment variables that have larger predictive power on the outcome, one possibility is to choose V so that the mean squared prediction error of the outcome variable is minimized in the pre-treatment period (see Abadie and Gardeazabal, 2003).

In particular, one can implement an iterative optimization procedure that searches among all (diagonal) positive semidefinite V-matrices and sets of w^*-weights for the best fitting convex combination of the control units, where "best fitting" refers to the fit between the outcome of the treated unit and of its synthetic control before the treatment takes place (see Abadie, Diamond, and Hainmueller, 2010).[3] Furthermore, the deviation from condition (5) imposed by this implementation process can be easily assessed, and could be shown as a complementary output of the analysis.

In other words, the synthetic control algorithm estimates the missing counterfactual as a weighted average of the outcomes of potential controls. The weights are chosen so that the pre-treatment outcome and the covariates of the synthetic control are, on average, very similar to those of the treated country. This approach comes with the evident advantages of *transparency* (as the weights W^* identify the countries that are used to estimate the counterfactual outcome of the country that liberalized the economy) and *flexibility* (as the set I_C of potential controls can be appropriately restricted to make the underlying country

[3]In principle, one could choose V without assigning any particular weight to the different covariates (e.g., using the Euclidean or Mahalanobis distance). However, in order to assign larger weights to the pre-treatment variables that have larger predictive power on the outcome, we follow Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) by choosing V so that the mean squared prediction error of the outcome variable (Y) is minimized in the pre-treatment period. In other words, the weights w^* are chosen so as to minimize the distance in the covariates space, but the distance metric is chosen so as to minimize the prediction error with respect to the outcome before the treatment. In a sense, the pre-treatment period ends up being a validation period, because the choice of the distance metric V is conditional on the minimization of the prediction error of the outcome over this exact period.

comparisons more sensible). Furthermore, the SCM rests on identification assumptions that are weaker than those required by estimators commonly applied in the trade-growth literature. For example, while panel models only control for confounding factors that are time invariant (fixed effect) or share a common trend (difference-in-differences), the model specified above allows the effect of unobservable confounding factors to vary with time. While the SCM can handle endogeneity due to (time-varying) omitted bias, however, it would still suffer from reverse causation if the timing of economic liberalization were decided by expectations on future growth prospects.

A limitation of the SCM is that it does not allow to assess the significance of the results using standard (large-sample) inferential techniques, because the number of observations in the control pool and the number of periods covered by the sample are usually quite small in comparative case studies like ours. As suggested by Abadie, Diamond, and Hainmueller (2010), however, placebo experiments based on permutation techniques can be implemented to make inference. Following their approach, we implement *cross-sectional* placebo tests; that is, we sequentially apply the synthetic control algorithm to every country in the pool of potential controls and compare the placebo with the baseline results. In other words, for each of the I_C potential controls, we estimate the dynamic treatment effects τ_{it} including the true treated economy in the donor pool and using the SCM as explained above. We then compare these effects with those estimated for the true treated economy. This is meant to assess whether the baseline estimates for the treated country are large relative to the effects for countries chosen at random.

5 Case Study Selection

Using the SCM to implement a set of comparative case studies and investigate the effect of economic liberalization on per capita income paths in eligible economies around the world requires, as a preliminary step, the identification of a pool of feasible experiments, that is, liberalization episodes that meet the following conditions: (i) the treated country liberalized at the earliest in 1965, as we require a few pre-liberalization observations to calibrate the synthetic control; and (ii) there exists a sufficient set of countries in the same region that remain closed for 10 years past the liberalization episode (or until the end of the sample) to effectively provide a pool of potential comparison economies that are "similar." To account for this similarity, which includes factors such as cultural proximity (but also stage of economic development in a broad sense), we group the countries by geographic region: Sub-Saharan Africa, Asia, Latin America, and the Middle East and North Africa. Given the above requirements, we are not able to analyze OECD countries, because the pool of potential comparison economies within this group is essentially empty and countries from other regions do not match up well in terms of GDP per capita.[4]

Tables 1 through 4 provide the full picture of liberalization episodes in the regions covered by our sample. As we can see for instance in Table 2, sweeping "waves of liberalization" are bad for our approach: We are quickly running out of potential comparison countries in Latin America as the trend toward greater economic liberalization essentially eliminates the intra-regional control group by the end of the 1980s.

Based on the liberalization sequences shown in the above tables, we are able to perform 5 comparative case studies in Asia, 5 in Latin America, 16 in Africa, and 4 in the Middle East and North Africa, for a total of 30 experiments.[5]

Finally, we choose the pool of potential comparison countries so as to perform two

[4]We also do not consider former socialist countries, because we have analyzed the specific problems raised by transitions away from a socialist economic system in Billmeier and Nannicini (2011).

[5]Specifically, we end up with the following eligible treated countries by region (year of economic liberalization in parentheses). Asia: Singapore (1965), South Korea (1968), Indonesia (1970), Philippines (1988), Nepal (1991). Latin America: Barbados (1966), Chile (1976), Colombia (1986), Costa Rica (1986), Mexico (1986). Sub-Saharan Africa: Mauritius (1968), Botswana (1979), Gambia (1985), Ghana (1985), Guinea (1986), Guinea-Bissau (1987), Mali (1988), Uganda (1988), Benin (1990), South Africa (1991), Cape Verde (1991), Zambia (1993), Cameroon (1993), Kenya (1993), Ivory Coast (1994), Niger (1994). Middle East and North Africa: Morocco (1984), Tunisia (1989), Mauritania (1995), Egypt (1995).

different types of SCM experiment. In what we call "type-A experiment," we allow the synthetic control algorithm to pick any eligible economy in the same region of the liberalizing country as a control. In "type-B experiments," instead, we increase the number of potential comparison units to include eligible economies from the other (developing) regions covered by our sample. Our final sample is therefore made up of 127 (treated and comparison) countries; for each of them, we observe the (non-missing) time series of per capita GDP from 1963 to 2005. In the Online Appendix, for transparency, we list all potential comparison countries included in each experiment. The trade-off between the two types of experiment is clear. Type-A experiments aim at excluding rather far-fetched country comparisons by ensuring a common support between treated and comparison countries with respect to factors related to geography and possibly cultural proximity, while type-B experiments increase sample size and the power of the test. We believe that exploiting the flexibility of the SCM and providing both types of results is useful to assess the robustness of our conclusions.

6 Economic Liberalization Episodes

In this section, we present and discuss the implemented experiments. In a first step, we reflect on the results by region, highlighting specific countries of interest. We report the results both numerically (Tables 5 through 10) and graphically (Figures 1 through 6). The tables provide the numerical comparison by explanatory variable between each treated country and the constructed synthetic control. As discussed in Section 5, "synthetic control A" refers to the estimated counterfactual composed of a pool of countries in the same region, "synthetic control B" to a worldwide donor pool. The overall pre-treatment fit is measured by the root mean square prediction error (RMSPE) of real GDP per capita. The selected covariates are included as averages in the algorithm so as to maximize the sample size, because some of them have missing values in many years. Following Abadie, Diamond, and Hainmueller (2010), each annual observation of the pre-treatment outcome variable (real GDP per capita) is instead used as a separate control variable in order to improve the pre-treatment fit.[6] The comparisons between the post-treatment outcome of the treated unit and the synthetic control after five (GDP at T_0+5) and ten years (GDP at T_0+10) provide the baseline estimates of two of the dynamic treatment effects.[7]

The figures, instead, represent graphically the time series of the outcome variable, real GDP per capita, for the treated unit (solid line) and the synthetic control unit (dashed line), both in the entire pre-treatment period and for ten years after the treatment year (T_0). The comparison between the solid and dashed line before T_0 captures the quality of the pre-treatment fit reached by the SCM algorithm; the same comparison after T_0 provides the (ten) dynamic treatment effects. Note that, in the tables, we report the estimation results of all the experiments (type-A and type-B), but—to contain space—we only show the figure of one experiment (A or B) per country. Our formal criterion to decide which experiment to present graphically is as follows: If the RMSPE for experiment A is smaller than 40 or smaller than experiment B, we show the evidence under experiment A, otherwise we use the alternative control sample, that is, experiment B.[8]

In a second step, we further investigate several liberalization episodes by means of placebo tests to check the robustness of our baseline results. The placebo experiments are contained in Figures 7 and 8 and discussed in the context of the country-specific results. We

[6]In the tables, "pre-treatment GDP" is averaged over the entire pre-treatment period just to provide a simple reference, but the algorithm minimizes the distance between each yearly value of GDP for the treated country and its synthetic control.

[7]We acknowledge that economic reforms do not always happen overnight; sometimes they are the marginal result of a gradual shift toward more market-friendly policies. However, this measurement error, inherent in the use of the SWWW indicator, would introduce a simple *attenuation bias* in our results, as the effect detected by the SCM would be lower if reforms were diluted across multiple years. Moreover, as the SCM can only be implemented with binary treatments, we believe that the advantages of this method discussed in Section 4 are worth the price in terms of measurement error.

[8]While the criterion is, of course, somewhat arbitrary, the figures for the alternative experiment in each country are available upon request. They are broadly consistent with the ones reported in this section, as can be also seen from the (complete) estimation results reported in Tables 5 through 10.

have chosen to provide this robustness analysis for countries where the primary evidence points to a significant impact of economic liberalization. In some examples, the placebo exercises in fact confirm and reinforce the evidence, whereas in other cases the placebo analysis reveals that the effect found in the initial assessment is rather coincidental.

Regarding the potential of reform heterogeneity, and to provide some contextual background, we discuss the broader reform agenda in one country per region somewhat more in depth; the analysis is mainly based on IMF (various). We conclude with a discussion on the interpretation of the evidence provided by our synthetic control experiments.

6.1 Asia

The results for Asia are presented graphically in Figure 1 (SCM results) and Figure 7 (placebo tests). Indonesia (treatment in 1970) is a prime example of economic liberalization gone well. The average income over the years before liberalization is literally identical to that of the synthetic control, which consists of Bangladesh (41 percent), India (23 percent), Nepal (23 percent), and Papua New Guinea (13 percent).[9] After the economic liberalization in 1970, however, Indonesian GDP per capita takes off and is 40 percent higher than the estimated counterfactual after only five years and 76 percent higher after ten years (see Table 5). The results for Indonesia are also strongly robust to placebo testing, as none of the "fake" experiments for the 8 (regional) potential comparison countries shows treatment effects larger than the baseline estimates.

The economic reform backdrop in Indonesia during 1970 (and 1971) is characterized by a strong (and successful) effort to reduce inflation. While the government already ran a balanced budget, it put emphasis on improving tax assessment and collection to expand tax revenues. Notwithstanding a substantial decrease in rubber prices, a staple export,

[9] See the Online Appendix for the complete list of potential comparison countries, as well as for the control units actually included by the algorithm in each synthetic control (with their relative weights).

the Indonesian current account improved on the back of export promotion measures and wood-related exports. From a liberalization perspective, the main event was the exchange reform introduced in April 1970. However, Indonesia continued to maintain a 10 percent export tax, albeit with numerous exceptions.

For the other four liberalization episodes in Asia, the intra-regional match is not particularly good (see the RMSPEs in Table 5). This shows why it is useful to enlarge the pool of potential control economies to the worldwide sample of closed economies (type-B experiment), as this step helps to regain comparable GDP levels in the comparison countries. Figure 5 shows that South Korea (1968) is a success story similar to Indonesia with income about twice as high as in the counterfactual case after 10 years. For Singapore (1965), we are tempted to argue that liberalization also worked, notwithstanding the fact that the counterfactual immediately after liberalization is performing better than Singapore. Note especially that GDP per capita in the synthetic control (a convex combination of Algeria and Mexico) continues to grow rather linearly, whereas in Singapore the path of GDP steepens drastically between 1965 and 1967. For both countries, the placebo test confirms the validity of the SCM results, as none of the 42 fake experiments show stronger results than the baseline.[10]

On the other hand, the later liberalization episodes—Philippines (1988) and Nepal (1991)—did either not lead to a significantly better trajectory than in the estimated counterfactual (in the case of the Philippines), or it is not clear to what extent the 30-percent income difference after 10 years in favor of the liberalized economy (Nepal) is attributable to the economic liberalization as (i) the steep income increase already starts a couple of years before the liberalization year according to the SWWW indicator, and (ii) the placebo test for Nepal reported in Figure 7 is not particularly robust.[11]

[10] The number of potential comparison countries, and therefore of placebo exercises, is the same for South Korea and Singapore because they both liberalized in 1965.

[11] As a matter of fact, although the placebo test for Nepal is clearly less robust than those for Indonesia, Singapore, and South Korea, it should be noted that only 3 out of 20 placebo units (15%) showed a higher

6.2 Latin America

Graphical evidence on the liberalization episodes in Latin America is shown in Figure 2 (SCM results) and Figure 7 (placebo tests). In this region, economic liberalization episodes that can be analyzed in our framework took place rather early, that is, between 1966 (Barbados) and 1986 (Colombia, Costa Rica, and Mexico). In all countries, the regional synthetic controls (type-A experiment) provide a fine match: On average, over the years before liberalization, the income of the synthetic control diverges by less than two percent from the liberalizing country's income (see Table 6); the only exception being Chile (1976), where the pre-liberalization drop in GDP (coinciding with the Pinochet coup) makes it difficult for the SCM algorithm to find a suitable counterfactual and reduces the inferential value of this experiment because of the poor pre-treatment fit.

In Chile, the implementation of the SCM is hampered by the exceptional events around the Pinochet coup in September 1973. Yet, some lessons can be learned also from this country episode. During the "Unidad Popular" government (1970-73), the state assumed a dominating role in the economy, including by controlling prices, interest rates, credit, and capital movements. Expansionary fiscal policies—including due to a strongly rising public payroll—led to a hefty increase in the budget deficit (on the order of 20 percent of GDP in 1973). After the 1973 coup, the widespread removal of price controls provoked a surge to triple-digit inflation rates and, reinforced by the wage adjustment formula, a wage-price spiral during the following three years. Chile's position was also weakened by the sharp downturn in world copper prices in 1974 and 1975. Over the 1973–75 period, Chile's output fell dramatically, and unemployment rose from about 3 percent to around 18 percent as public sector employment suffered from significant demand retrenchment. In 1976, Chile started to rebound helped by rising copper prices, and embarked on second-

treatment effect than Nepal; it is only because of a scale effect (the above three economies being much richer than Nepal) that this fact overshadows the other (robust) 17 placebo experiments in Figure 7.

round economic liberalization measures geared toward the external sector, which included rationalizing the import duty system and lowering the marginal rates, while also reducing non-tariff barriers and removing the multiple exchange rate system. Looking at Figure 2, it could be argued that the economic policy measures taken in 1975–76, including economic liberalization, turned income growth around and put it on a parallel track to the estimated synthetic control (which mainly consists of Uruguay and Honduras).

Barbados, Colombia, Costa Rica, and Mexico are instead excellent examples of a positive and robust impact of economic liberalization (see both Figure 2 and Table 6). Ten years after liberalization, GDP per capita is about 57 percent higher than that of the regional synthetic control in Barbados, 23 percent in Colombia, 26 percent in Costa Rica, and 21 percent in Mexico. The placebo tests confirm that the SCM results are largely robust for these countries (see Figure 7). For Barbados and Mexico, none of the fake experiments in the potential controls is above the effect in the treated country, while this happens for only 1 case (out of 24) in Colombia and 1 case (out of 9) in Costa Rica.

Summing up, the feasible country experiments in Latin America—which mainly took place in the 1980s—lead to the conclusion that economic liberalization had a positive impact on income per capita.

6.3 Africa

The 16 economic liberalization episodes that we can analyze in Africa under the SCM framework occurred between 1968 (Mauritius) and 1994 (Ivory Coast and Niger). See Figures 3 through 5 for the SCM evidence, and Figure 8 for selected placebo tests. From the analysis of the African subsample, we draw two conclusions.

First, the empirical evidence seems to indicate four different groups: (1) treated countries outperforming the estimated counterfactual with strong support from the placebo tests, (2) outperformance with some support from the placebo tests, (3) strong economic rebound but without significant outperformance, and (4) limited effect. Botswana is the only country clearly in the first group, offering a truly convincing success story.[12] In particular, Botswana fared substantially better over the ten years after the liberalization (1979) than the synthetic control in the type-B experiment (see Figure 3), and this is also confirmed by the type-A experiment (see Table 7). In fact, the income per capita in Botswana is about five times as high as the one of the synthetic control ten years after liberalizing the economy. This preliminary conclusion is strongly reinforced by the placebo test (see Figure 8): The bold line is higher than any other of the 47 permutations, indicating that the baseline results are not driven by random chance and capture the true treatment effect of economic liberalization in this country.

Countries in the second group have outperformed the respective counterfactual, and the placebo tests (see Figure 8) lend good support to this conclusion as only very few placebo permutations are above the treated country 10 years after liberalization. This group includes Benin (3 out of 19 permutations above the treated economy), Ghana (3/24), Guinea (5/24), Guinea-Bissau (4/21), Mauritius (5/62), and Uganda (5/21).[13] In the late 1980s, Benin began a process of general economic liberalization after many years of political oppression, which had left the country in ruins: A collapsed banking system, public enterprises under distress, large internal and external financing gaps, and arrears on both accounts. This situation led to political unrest and strikes, triggering a national convention in February 1990, which led, in turn, to free elections in early 1991. The economic program adopted by the national convention aimed at encouraging private sector activity, reducing internal and external imbalances, containing cost pressures via strong demand manage-

[12]We refrain from commenting further on Botswana as its success has been well documented in the literature. See Acemoglu *et al.* (2003), who point to the interaction between solid institutions and sound economic policy as key ingredients of the country's economic success.

[13]Note that the key information content of the placebo exercises resides in the number of dashed lines above the bold one, not in the magnitude or difference between the true and the fake effects. In other words, a result is more significant in the placebo-test sense if only very few dashed lines are above the treated country, even if by a lot, as opposed to many dashed lines by a small amount.

ment and a reduction of administrative measures and the role of the state in the economy more broadly (including via privatization of state-owned enterprises). Good progress in initiating structural reforms was initially hampered by the severe financial crisis, but the economy turned around in 1990 and recorded positive growth. The main measures from a trade liberalization perspective included eliminating the import license requirements for goods from certain countries, as well as the renewal of trading licenses for commercial importers. Moreover, a program for tariff reform was introduced.

The third group—characterized by strong rebounds (or expansions) in income per capita around the liberalization year but without outperformance of the counterfactual—contains Mali, Cape Verde, and possibly Ivory Coast. Given the mixed empirical message and to contain space, we refrain from showing the corresponding placebo tests. Finally, the fourth group contains Cameroon, Gambia, Kenya, Niger, South Africa, and Zambia as well as Ivory Coast to the extent that this country seems to be a candidate for both group 3 and group 4. None of these countries seems to have benefited from economic liberalization: Real income per capita either stagnated or even declined over the ten years after economic liberalization.

The second conclusion we draw—restating the previous point regarding group 4 differently—is that the positive evidence seems to be concentrated in the first part of the sample, while the effect of economic liberalization in Africa has little to no discernible positive effect after around 1990. Ever since, liberalization either stops a decline in income whereas the counterfactual grows steadily during the post-liberalization period (e.g., Cameroon, Niger), or has no apparent impact on income levels (Kenya, South Africa).[14] In Zambia, even after economic liberalization in 1993, the income level continued to decline on a per-capita basis. One somewhat positive example of a rather late liberalization is Cape Verde

[14] An earlier reformer, Gambia, also displays this pattern; we consider Gambia a special case however because the volatility of per-capita income right before liberalization makes the construction of the counterfactual particularly difficult, therefore reducing the inferential value of this experiment.

(1991), with income per capita about 15 percent higher than that of the synthetic control after ten years. That said, the divergence from the counterfactual is very slow to emerge, weakening the causal link to economic liberalization.[15]

Summing up, in sub-Saharan Africa, it seems to broadly be the case that only early liberalizations had a positive impact on income per capita, while almost all of the late attempts did not benefit the liberalizing country much. An alternative explanation of the zero findings in the 1990s, however, might be that late-liberalizers in Africa adopted gradual reform strategies, leading to attenuation bias in the results.[16]

6.4 Middle East and North Africa

The results for the Middle East and North Africa (MENA) region are graphically summarized in Figure 6 (SCM results) and Figure 8 (placebo tests). In this region, the results are far from conclusive. In all countries, the difference in GDP per capita between the liberalizing economy and the synthetic control constrained to the region is quite small at the time of treatment. But after ten years, only in Morocco (1984) liberalization has contributed to a somewhat higher income level than in the synthetic control (see Table 10). This positive effect, however, is not particularly robust to placebo testing, as 4 out of 11 permutations are either above or essentially identical with the baseline effect in the treated country. Since the late 1970s, Morocco had built up large internal and external imbalances due to expansionary policies in the context of deteriorating terms of trade, bad harvests, and structural weaknesses in the economy. Under the auspices of an IMF

[15] Note that the type-B experiment for Cape Verde provides a particularly far-fetched county match, as the estimated counterfactual is mainly composed of China. Results should thus be interpreted with care. We believe, however, that this exemplifies one of the advantages of the SCM identified above. By being able to identify the countries actually chosen by the algorithm, we can discuss the meaningfulness and robustness of each empirical exercise using additional, case study-type considerations. Other estimators commonly used in the literature might well be based on the same country comparisons or—even worse—on parametric extrapolation, but often do not make these comparisons explicit, which therefore cannot be assessed (see Billmeier and Nannicini, 2009).

[16] See the discussion in footnote 7.

program, the country made good progress in reducing the balance of payments gap by sharply curtailing government spending and by implementing tight incomes and credit policies in 1984. Moreover, the country also pursued a substantial trade and price liberalization. Apart from the reduction in import tax and tariffs, the import restrictions which had been severely tightened in the previous year were eased in January and July 1984. As a result, the share of unrestricted imports in total imports rose to almost 50 percent. In addition, economic liberalization also included eliminating the import deposit scheme and the state export monopoly for processed foods as well as the export licensing requirement for industrial and most agricultural products.

In the three other countries where liberalization took place rather late (Tunisia 1989; Mauritania 1995; Egypt 1995), however, the liberalizing country actually fares worse than the regional synthetic control both five and ten years after liberalization.

6.5 What Changed in the 1990s?

The analysis in the preceding sections indicates that economic liberalization is, in some countries, associated with a remarkable positive effect on real income. However, we find a lot of heterogeneity in the results across regions and time. In particular, we note that countries that liberalized their economy after about 1990—many of which are located in Africa—did not benefit from these reforms in terms of higher GDP per capita compared to similar, but closed economies. Why is this?

We speculate that one explanation could reside in a *timing* effect of economic liberalization. In our view, this timing effect is slightly different, but possibly related to the ones found in the literature by Giavazzi and Tabellini (2005), who document that countries that liberalize the economy before becoming democracies do substantially better than those that follow the opposite track, and by Wood (1997), who shows a time effect in the interaction between trade openness and wage inequality.[17] In our view, the timing argument could also work as follows: If a developing economy liberalizes early on, especially in terms of trade openness, it can still reap the gains of specialization, while once "globalization" kicks in (and an increasing number of countries liberalize their economy), there is much more competition for capital, but also for the labor-intensive goods a developing economy can specialize in, e.g., agriculture or textiles.[18] Hence, the benefits from liberalization after globalization are smaller.[19] The SCM evidence presented above is consistent with the history of economic liberalization in the developing world: Asian economies liberalized early on, and this allowed them to benefit from their comparative advantage in labor-intensive goods. Once Latin American countries also started to liberalize, the Asian economies started shifting their comparative advantage to more capital-intensive production and higher-value exports (Weiss, 2005). Finally, once the liberalization wave hit Africa, the benefits from joining the club of liberalized economies had become smaller, as other countries that had liberalized somewhat earlier did not move up the specialization ladder.

The above timing argument, however, is not the only possible interpretation of our empirical results. A second explanation—which is again speculative but broadly consistent

[17] Focusing on the skilled versus unskilled labor content of trade, Wood (1997) shows that wage inequality shrinks in East Asia in the 1960s and 1970s with increasing international trade—consistent with the conventional wisdom *à la* Heckscher-Ohlin—while inequality rises with trade in Latin America in the 1980s and early 1990s. He ascribes the differences mainly to temporal factors (e.g., entry of China in the global labor market, technological progress biased against unskilled workers) rather than geographical differences (e.g., resource endowments, different trade policies).

[18] We sidestep a full-blown discussion of the globalization phenomenon here as this goes beyond the scope of this paper. We note however, that a narrow measure of globalization, e.g., trade flows, would not capture more recent aspects of globalization that accelerated in the late 1980s–early 1990s, such as the increasing access to and use of telecommunications, and the internet. Almost by definition, there is no good measure for globalization over long periods of time as some of the very factors that shape it only become available over time. See Bhandari and Heshmati (2005) for an attempt to quantify globalization over the (short) period 1995–2001.

[19] From a theoretical perspective, this effect is not visible in a simple two-country model where countries do not compete for capital and export demand from a third country. In a multilateral model, however, the timing of liberalization becomes crucial, especially if the specialization paths are not complementary (see Balassa, 1979).

with the SCM evidence—is that economic liberalizations that occurred later lacked the beneficial interaction with other growth-enhancing fundamentals, such as institutional quality. While the political economy literature—see for example Acemoglu *et al.* (2001)—argues that good institutions will lead to good policies, which, in turn, will cause good outcomes, we show here that good policies do not always work. This constitutes, however, not necessarily a contradiction as some good policies (e.g., economic liberalization) could still lack the positive interaction with other favorable and complementary policies, such as investment in physical and human capital or property rights protection (e.g., see Rodrik, 1999, 2005), because of low-quality institutions.[20]

7 Conclusion

In this paper, we explore the effect of economic liberalization on income per capita. We investigate this question in as large a set of countries as admissible given our econometric strategy. The basic question we ask is: Do economies that have experienced economic liberalization grow faster than those that have not? Our estimator, drawn from the treatment evaluation literature, establishes a middle ground between large-sample cross-country studies and descriptive case studies. This methodology—the synthetic control method—compares a treated (liberalized) country with an estimated (closed) counterfactual. The peculiarity of this method rests in the fact that the counterfactual is a linear combination of comparison units that are similar to the treated economy along covariates traditionally used in the literature and pre-treatment realizations of the outcome variable.

Starting from a worldwide sample of countries, we devise a case study selection strat-

[20]Rodrik (2007) makes a similar point: The "new conventional wisdom" on globalization points to a range of institutional complements in developed and developing economies to deliver the benefits of globalization and remain sustainable by consolidating progress made so far and garnering further support in the public eye. In particular, this would entail reforms to the social safety nets (to ease adjustment and enable redistribution of globalization benefits) and, in developing economies, more basic institutional reforms including anti-corruption, labor, and financial markets.

egy that first focuses on liberalized economies with a sufficient pool of regional comparison countries that have not liberalized. We then broaden the pool of eligible controls to improve the pre-treatment fit between the treated country and the estimated counterfactual. We find that economic liberalization (as represented by the updated Sachs-Warner indicator) tends to have, by and large, a positive—or at least nonnegative—impact on the trajectory of real income per capita.

We also find, however, that the benefit of economic liberalization tends to be higher for countries that liberalized before the onset of the latest wave of globalization. Especially in sub-Saharan Africa and in the MENA region, where a number of liberalization episodes took place in the 1990s, we show that the income differential between the treated country and the estimated counterfactual is either small or not robust to placebo tests for those economies that lagged behind.

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Tables and Figures

Table 1: Economic Liberalizations in Asia

Country	*Treatment Status*
Hong Kong, SAR	always open
Thailand	always open
Malaysia	open since 1963
Taiwan, Province of China	open since 1963
Singapore	open since 1965
Korea (Republic of)	open since 1968
Indonesia	open since 1970
Sri Lanka	open since 1977 (waves after)
Phillippines	open since 1988
Nepal	open since 1991
Bangladesh	open since 1996
China PR	always closed
India	always closed
Pakistan	always closed
Papua New Guinea	always closed
Afghanistan	not available
Bhutan	not available
Brunei	not available
Cambodia	not available
Fiji	not available
Korea (Democratic People's Republic of)	not available
Laos	not available
Mongolia	not available
Myanmar (Burma)	not available
Samoa (Western)	not available
Solomon Islands	not available
Tonga	not available
Vanuatu	not available
Vietnam	not available

Source: Sachs and Warner (1995); Wacziarg and Welch (2003, 2008).

Table 2: Economic Liberalizations in Latin America

Country	*Treatment Status*
Bolivia	always open (except 1979-84)
Ecuador	always open (except 1982-90)
Barbados	open since 1966
Chile	open since 1976
Colombia	open since 1986
Costa Rica	open since 1986
Mexico	open since 1986
Guatemala	open since 1988
Guyana	open since 1988
El Salvador	open since 1989
Jamaica	open since 1989 (waves before)
Paraguay	open since 1989
Venezuela	open since 1989 (waves after)
Uruguay	open since 1990
Argentina	open since 1991
Brazil	open since 1991
Honduras	open since 1991
Nicaragua	open since 1991
Peru	open since 1991
Dominican Republic	open since 1992
Trinidad & Tobago	open since 1992
Panama	open since 1996
Haiti	always closed
Antigua	not available
Bahamas	not available
Belize	not available
Cuba	not available
Dominica	not available
Grenada	not available
St. Kitts & Nevis	not available
St. Lucia	not available
St. Vincent & Grenadines	not available
Suriname	not available

Source: Sachs and Warner (1995); Wacziarg and Welch (2003, 2008).

Table 3: Economic Liberalizations in Africa

Country	*Treatment Status*
Mauritius	open since 1968
Botswana	open since 1979
Gambia	open since 1985
Ghana	open since 1985
Guinea	open since 1986
Guinea Bissau	open since 1987
Mali	open since 1988
Uganda	open since 1988
Benin	open since 1990
Cape Verde	open since 1991
South Africa	open since 1991
Cameroon	open since 1993
Kenya	open since 1993
Zambia	open since 1993
Ivory Coast	open since 1994
Niger	open since 1994
Mozambique	open since 1995
Tanzania	open since 1995
Ethiopia	open since 1996
Madagascar	open since 1996
Burkina Faso	open since 1998
Burundi	open since 1999
Angola	always closed
Central African Republic	always closed
Chad	always closed
Congo	always closed
Gabon	always closed
Lesotho	always closed
Malawi	always closed
Nigeria	always closed
Rwanda	always closed
Senegal	always closed
Sierra Leone	always closed
Togo	always closed
Zimbabwe	always closed
Comoros	not available
Djibouti	not available
Equatorial Guinea	not available
Eritrea	not available
Liberia	not available
Namibia	not available
Sao Tome & Principe	not available
Seychelles	not available
Somalia	not available
Sudan	not available
Swaziland	not available
Zaire	not available

Source: Sachs and Warner (1995); Wacziarg and Welch (2003, 2008).

Table 4: Economic Liberalizations in the Middle East

Country	*Treatment Status*
Yemen	always open
Jordan	open since 1965
Morocco	open since 1984
Tunisia	open since 1989
Egypt	open since 1995
Mauritania	open since 1995
Algeria	always closed
Iran	always closed
Iraq	always closed
Syria	always closed
Bahrain	missing
Kuwait	missing
Lebanon	missing
Libya	missing
Oman	missing
Qatar	missing
Saudi Arabia	missing
United Arab Emirates	missing

Source: Sachs and Warner (1995); Wacziarg and Welch (2003, 2008).

Table 5: Covariates and Outcome Means — Asia

	Singapore 1965	Synth. Control A	Synth. Control B
Population growth	2.87	2.94	2.88
Pre-treatment GDP	2,569.49	612.54	2,573.46
GDP at $T_0 + 5$	3,901.08	693.39	3,270.89
GDP at $T_0 + 10$	6,143.02	791.80	3,801.23
RMSPE		1,958.21	0.00
	South Korea 1968	Synth. Control A	Synth. Control B
Secondary school	31.00	33.50	11.87
Population growth	2.62	3.05	2.76
Investment share	0.17	0.15	0.17
Pre-treatment GDP	1,290.43	631.62	1,290.46
GDP at $T_0 + 5$	2,045.33	731.28	1,612.73
GDP at $T_0 + 10$	3,008.49	890.37	1,572.70
RMSPE		663.59	12.82
	Indonesia 1970	Synth. Control A	Synth. Control B
Secondary school	9.00	11.30	7.81
Population growth	2.20	2.32	2.28
Investment share	0.07	0.10	0.09
Pre-treatment GDP	247.90	247.96	258.15
GDP at $T_0 + 5$	361.11	258.19	308.00
GDP at $T_0 + 10$	465.99	264.62	303.05
RMSPE		5.20	0.01
	Philippines 1988	Synth. Control A	Synth. Control B
Secondary school	57.80	9.88	32.84
Population growth	2.74	2.31	2.99
Investment share	0.16	0.13	0.11
Inflation	11.34	7.84	8.89
Democracy	0.50	1.00	0.04
Pre-treatment GDP	794.88	507.82	802.20
GDP at $T_0 + 5$	848.96	581.68	945.83
GDP at $T_0 + 10$	949.28	582.53	977.31
RMSPE		303.00	35.74
	Nepal 1991	Synth. Control A	Synth. Control B
Secondary school	21.11	33.61	5.13
Population growth	2.34	2.22	2.79
Investment share	0.10	0.12	0.14
Inflation	8.47	7.81	14.36
Democracy	0.03	1.00	0.00
Pre-treatment GDP	159.81	225.23	162.65
GDP at $T_0 + 5$	192.78	383.81	167.28
GDP at $T_0 + 10$	234.59	460.82	179.75
RMSPE		78.28	17.94

Source: authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of the covariates and outcome variables. Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. The value of each predictor is averaged over the pre-treatment period. The values of the outcome refer to five years (T_0+5) and ten years (T_0+10) after the treatment year T_0. RMSPE stands for Root Mean Squared Prediction Error. Synthetic control A is constructed from a pool of potential controls including only Asian countries; synthetic control B is constructed from a worldwide pool of potential controls. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Table 6: Covariates and Outcome Means — Latin America

	Barbados 1966	Synth. Control A	Synth. Control B
Secondary school	45.50	23.54	14.00
Population growth	0.37	3.40	0.59
Investment share	0.15	0.29	0.16
Pre-treatment GDP	3,377.93	3,381.60	3,376.01
GDP at $T_0 + 5$	4,604.42	3,877.56	4,574.42
GDP at $T_0 + 10$	6,345.67	4,053.99	5,920.59
RMSPE		0.00	0.00
	Chile 1976	**Synth. Control A**	**Synth. Control B**
Secondary school	35.70	41.61	42.07
Population growth	2.06	1.27	1.23
Investment share	0.17	0.12	0.12
Democracy	0.81	0.62	0.64
Pre-treatment GDP	2,069.70	2,067.31	2,068.03
GDP at $T_0 + 5$	2,331.43	2,814.71	2,810.66
GDP at $T_0 + 10$	2,061.61	2,431.42	2,426.75
RMSPE		84.51	84.49
	Colombia 1986	**Synth. Control A**	**Synth. Control B**
Secondary school	33.50	27.60	34.18
Population growth	2.54	2.97	3.03
Investment share	0.13	0.16	0.14
Inflation	17.60	40.24	7.44
Democracy	1.00	0.23	0.09
Pre-treatment GDP	1,262.72	1,282.51	1,260.06
GDP at $T_0 + 5$	1,718.01	1,475.12	1,354.10
GDP at $T_0 + 10$	1,947.22	1,581.01	1,569.98
RMSPE		45.35	35.21
	Costa Rica 1986	**Synth. Control A**	**Synth. Control B**
Secondary school	38.19	27.24	42.88
Population growth	3.29	2.58	2.61
Investment share	0.15	0.23	0.21
Inflation	12.87	72.83	5.91
Democracy	1.00	0.48	0.18
Pre-treatment GDP	2,767.40	2,758.52	2,744.17
GDP at $T_0 + 5$	3,232.27	2,793.82	2,754.89
GDP at $T_0 + 10$	3,708.23	2,945.76	3,037.53
RMSPE		115.58	187.98
	Mexico 1986	**Synth. Control A**	**Synth. Control B**
Secondary school	40.95	52.31	38.88
Population growth	2.86	1.74	2.24
Investment share	0.20	0.16	0.18
Inflation	20.30	41.87	6.81
Democracy	0.00	0.76	0.12
Pre-treatment GDP	4,331.43	4,444.00	4,013.55
GDP at $T_0 + 5$	5,461.90	4,258.11	3,793.08
GDP at $T_0 + 10$	5,380.47	4,452.21	4,047.28
RMSPE		254.40	947.43

Source: authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of the covariates and outcome variables. Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. The value of each predictor is averaged over the pre-treatment period. The values of the outcome refer to five years ($T_0 + 5$) and ten years ($T_0 + 10$) after the treatment year T_0. RMSPE stands for Root Mean Squared Prediction Error. Synthetic control A is constructed from a pool of potential controls including only Latin American countries; synthetic control B is constructed from a worldwide pool of potential controls. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Table 7: Covariates and Outcome Means — Africa Before 1987

	Mauritius 1968	Synth. Control A	Synth. Control B
Secondary school	25.00	5.17	5.57
Population growth	2.51	2.70	2.39
Investment share	0.15	0.10	0.09
Pre-treatment GDP	917.09	914.67	917.12
GDP at $T_0 + 5$	898.80	945.07	873.38
GDP at $T_0 + 10$	1,322.95	867.21	973.66
RMSPE		45.89	25.45
	Botswana 1979	**Synth. Control A**	**Synth. Control B**
Secondary school	6.73	5.97	28.05
Population growth	1.26	2.69	2.22
Investment share	0.17	0.08	0.16
Democracy	1.00	0.31	0.00
Pre-treatment GDP	465.76	486.04	488.41
GDP at $T_0 + 5$	1,539.36	439.19	597.72
GDP at $T_0 + 10$	2,182.08	429.51	596.12
RMSPE		106.74	92.54
	Gambia 1985	**Synth. Control A**	**Synth. Control B**
Secondary school	10.73	5.50	19.75
Population growth	2.95	2.76	2.05
Investment share	0.03	0.14	0.10
Democracy	1.00	0.02	0.04
Pre-treatment GDP	208.25	198.21	210.66
GDP at $T_0 + 5$	242.98	203.98	356.05
GDP at $T_0 + 10$	244.43	193.50	471.19
RMSPE		13.08	10.22
	Ghana 1985	**Synth. Control A**	**Synth. Control B**
Population growth	2.47	2.43	2.42
Investment share	0.10	0.04	0.04
Democracy	0.16	0.02	0.02
Secondary school	30.31	9.76	9.51
Pre-treatment GDP	298.92	300.63	299.63
GDP at $T_0 + 5$	256.76	253.83	252.77
GDP at $T_0 + 10$	283.32	230.53	230.65
RMSPE		14.91	14.81
	Guinea 1986	**Synth. Control A**	**Synth. Control B**
Secondary school	12.44	10.65	10.14
Population growth	1.86	2.42	2.56
Investment share	0.11	0.09	0.10
Democracy	0.00	0.02	0.06
Pre-treatment GDP	316.63	318.88	316.64
GDP at $T_0 + 5$	342.61	334.71	321.87
GDP at $T_0 + 10$	345.17	307.98	335.91
RMSPE		2.80	2.60

Source: authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of the covariates and outcome variables. Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. The value of each predictor is averaged over the pre-treatment period. The values of the outcome refer to five years ($T_0 + 5$) and ten years ($T_0 + 10$) after the treatment year T_0. RMSPE stands for Root Mean Squared Prediction Error. Synthetic control A is constructed from a pool of potential controls including only African countries; synthetic control B is constructed from a worldwide pool of potential controls. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Table 8: Covariates and Outcome Means — Africa Between 1987 and 1991

	Guinea-Bissau 1987	Synth. Control A	Synth. Control B
Secondary school	7.25	6.21	8.79
Population growth	1.90	2.11	2.19
Investment share	0.13	0.06	0.10
Democracy	0.00	0.00	
Pre-treatment GDP	151.38	150.47	151.03
GDP at $T_0 + 5$	189.74	188.29	213.20
GDP at $T_0 + 10$	199.29	152.89	255.38
RMSPE		9.96	9.19
	Mali 1988	**Synth. Control A**	**Synth. Control B**
Secondary school	6.01	8.37	4.46
Population growth	2.15	2.33	2.12
Investment share	0.08	0.06	0.07
Democracy	0.00	0.05	0.05
Pre-treatment GDP	231.89	234.36	233.40
GDP at $T_0 + 5$	241.47	224.33	227.85
GDP at $T_0 + 10$	259.16	249.36	223.60
RMSPE		11.98	13.17
	Uganda 1988	**Synth. Control A**	**Synth. Control B**
Secondary school	7.31	11.18	3.35
Population growth	3.03	2.54	2.09
Investment share	0.02	0.04	0.08
Democracy	0.36	0.01	0.07
Pre-treatment GDP	198.23	200.92	200.97
GDP at $T_0 + 5$	162.68	152.82	219.99
GDP at $T_0 + 10$	205.59	165.06	219.99
RMSPE		12.48	17.25
	Benin 1990	**Synth. Control A**	**Synth. Control B**
Secondary school	13.66	15.11	13.50
Population growth	2.76	2.66	2.62
Investment share	0.06	0.08	0.09
Democracy	0.10	0.02	0.00
Pre-treatment GDP	372.66	374.91	371.83
GDP at $T_0 + 5$	355.08	333.35	315.38
GDP at $T_0 + 10$	395.34	348.76	333.64
RMSPE		13.47	12.39
	South Africa 1991	**Synth. Control A**	**Synth. Control B**
Secondary school	51.96	29.96	32.21
Population growth	2.35	2.89	3.03
Investment share	0.16	0.22	0.24
Democracy	1.00	0.71	0.54
Pre-treatment GDP	2,424.17	2,431.30	2,443.50
GDP at $T_0 + 5$	2,273.23	2,721.67	2,416.28
GDP at $T_0 + 10$	2,402.46	2,752.29	2,417.09
RMSPE		110.09	90.21
	Cape Verde 1991	**Synth. Control A**	**Synth. Control B**
Population growth	1.89	2.53	2.28
Investment share	0.17	0.13	0.16
Secondary school	12.87	23.03	35.20
Pre-treatment GDP	691.79	688.25	683.25
GDP at $T_0 + 5$	986.36	930.14	980.44
GDP at $T_0 + 10$	1,315.49	998.44	1,149.68
RMSPE		57.98	55.68

Source: authors' calculations based on data in Persson and Tabellini (2006). See the notes to Table 7.

Table 9: Covariates and Outcome Means — Africa After 1991

	Zambia 1993	Synth. Control A	Synth. Control B
Secondary school	17.84	17.52	18.33
Population growth	3.01	2.58	2.61
Investment share	0.14	0.06	0.07
Democracy	0.21	0.02	0.00
Pre-treatment GDP	576.04	577.92	578.98
GDP at $T_0 + 5$	345.84	439.88	442.80
GDP at $T_0 + 10$	358.90	427.91	409.70
RMSPE		45.23	45.13
	Cameroon 1993	**Synth. Control A**	**Synth. Control B**
Secondary school	20.04	19.67	43.68
Population growth	2.57	2.86	2.48
Investment share	0.08	0.09	0.15
Democracy	0.00	0.32	0.00
Pre-treatment GDP	681.22	700.96	681.48
GDP at $T_0 + 5$	620.22	1,410.83	1,146.56
GDP at $T_0 + 10$	670.92	1,416.98	1,321.51
RMSPE		94.00	91.93
	Kenya 1993	**Synth. Control A**	**Synth. Control B**
Secondary school	18.95	13.49	22.19
Population growth	3.42	2.92	2.81
Investment share	0.13	0.10	0.09
Democracy	0.10	0.10	0.05
Pre-treatment GDP	360.64	359.74	363.86
GDP at $T_0 + 5$	411.45	533.44	494.63
GDP at $T_0 + 10$	421.51	541.75	550.62
RMSPE		11.20	13.47
	Ivory Coast 1994	**Synth. Control A**	**Synth. Control B**
Secondary school	17.47	16.62	24.78
Population growth	3.67	2.17	2.30
Investment share	0.08	0.07	0.07
Democracy	0.00	0.21	0.16
Pre-treatment GDP	745.56	742.04	745.80
GDP at $T_0 + 5$	729.60	726.35	634.04
GDP at $T_0 + 10$	643.90	694.39	656.04
RMSPE		60.99	37.78
	Niger 1994	**Synth. Control A**	**Synth. Control B**
Secondary school	4.99	7.70	7.43
Population growth	3.12	2.85	2.85
Investment share	0.09	0.13	0.13
Democracy	0.09	0.00	0.00
Pre-treatment GDP	236.28	245.42	235.33
GDP at $T_0 + 5$	198.30	249.99	236.36
GDP at $T_0 + 10$	185.47	262.91	246.54
RMSPE		26.34	25.05

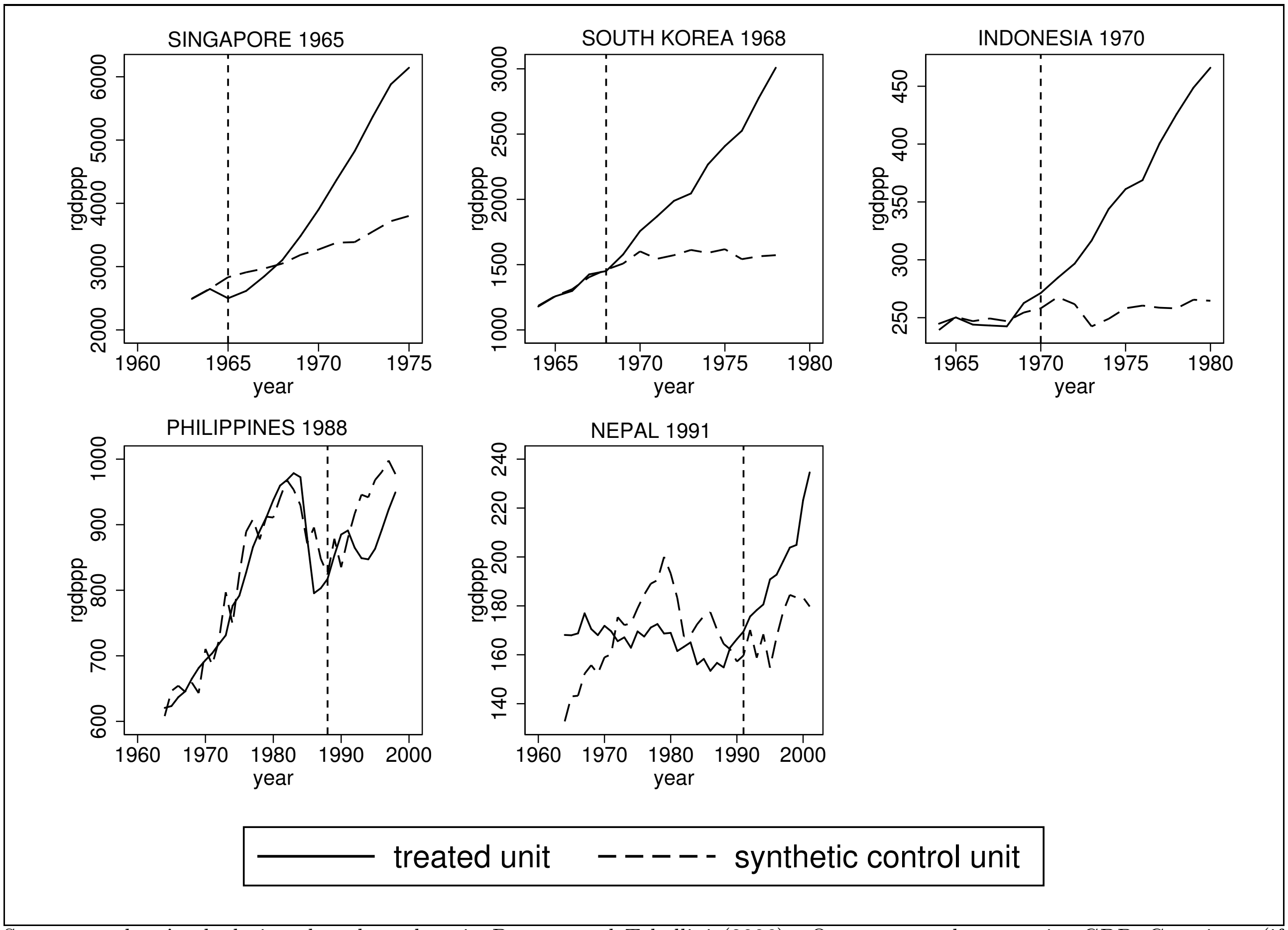
Source: authors' calculations based on data in Persson and Tabellini (2006). See the notes to Table 7.

Table 10: Covariates and Outcome Means — Middle East

	Morocco 1984	Synth. Control A	Synth. Control B
Secondary school	31.67	31.50	31.69
Population growth	2.49	2.91	2.49
Investment share	0.14	0.12	0.14
Pre-treatment GDP	825.47	823.18	811.31
GDP at $T_0 + 5$	1,078.58	881.46	961.98
GDP at $T_0 + 10$	1,098.18	879.01	1,040.32
RMSPE		30.87	25.71
	Tunisia 1989	**Synth. Control A**	**Synth. Control B**
Secondary school	41.05	51.90	37.95
Population growth	2.29	2.82	2.21
Investment share	0.18	0.10	0.18
Pre-treatment GDP	1,117.94	1,118.54	1,116.01
GDP at $T_0 + 5$	1,647.92	1,507.70	1,896.77
GDP at $T_0 + 10$	1,914.99	1,659.60	2,289.96
RMSPE		97.30	42.07
	Mauritania 1995	**Synth. Control A**	**Synth. Control B**
Secondary school	12.39	17.92	12.49
Population growth	2.47	2.55	2.48
Investment share	0.06	0.10	0.06
Pre-treatment GDP	400.23	406.47	398.32
GDP at $T_0 + 5$	437.83	445.63	451.69
GDP at $T_0 + 10$	476.77	552.70	491.75
RMSPE		29.05	13.91
	Egypt 1995	**Synth. Control A**	**Synth. Control B**
Secondary school	62.07	44.22	41.10
Population growth	2.31	2.91	2.12
Investment share	0.07	0.14	0.12
Pre-treatment GDP	764.58	787.82	768.19
GDP at $T_0 + 5$	1,254.94	964.21	1,366.50
GDP at $T_0 + 10$	1,388.62	1,026.88	1,518.83
RMSPE		85.45	48.33

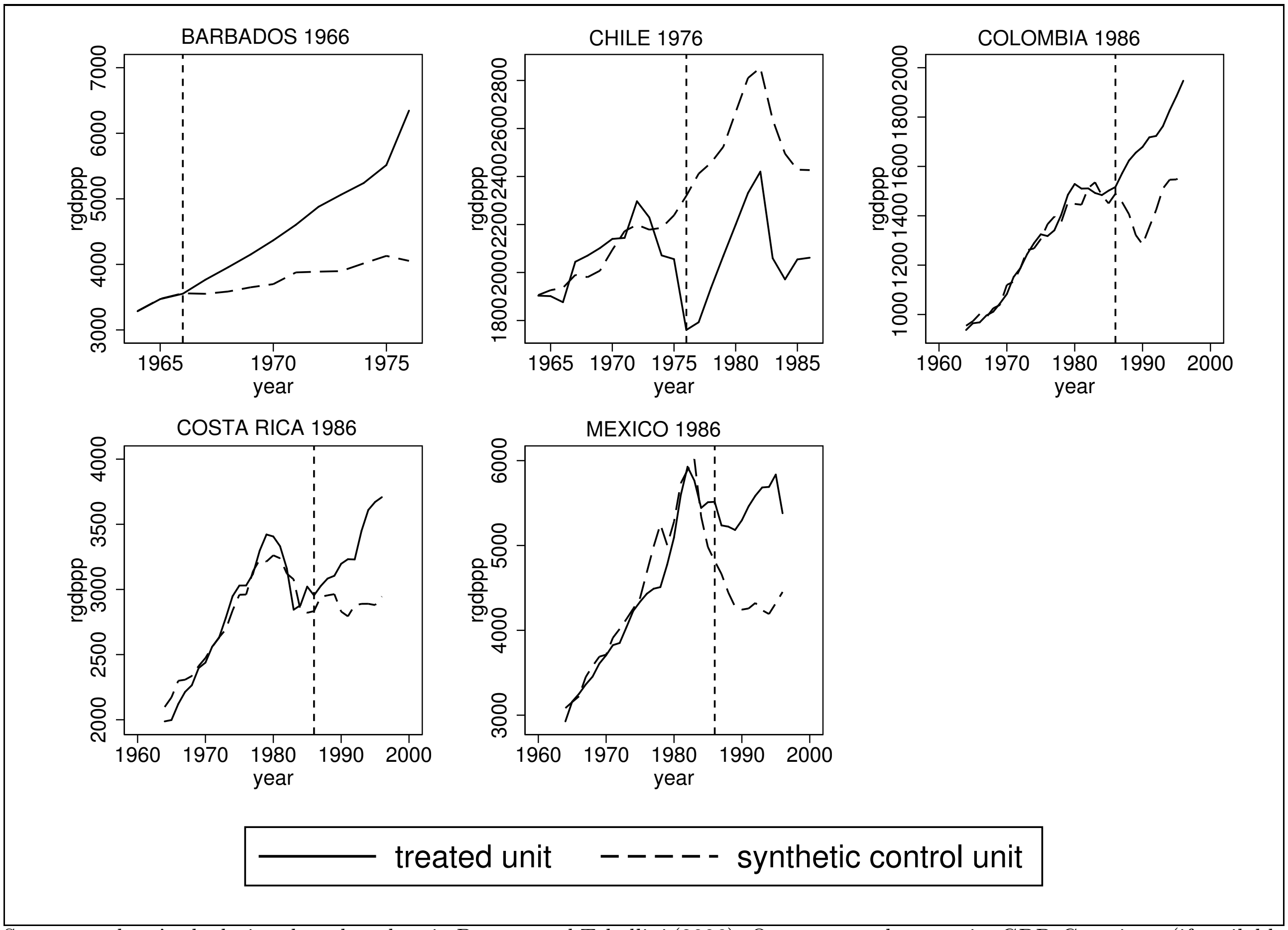
Source: authors' calculations based on data in Persson and Tabellini (2006). The table shows the mean values of the covariates and outcome variables. Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. The value of each predictor is averaged over the pre-treatment period. The values of the outcome refer to five years ($T_0 + 5$) and ten years ($T_0 + 10$) after the treatment year T_0. RMSPE stands for Root Mean Squared Prediction Error. Synthetic control A is constructed from a pool of potential controls including only countries in the Middle East; synthetic control B is constructed from a worldwide pool of potential controls. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 1: GDP Trends, Treated Country vs. Synthetic Control — Asia



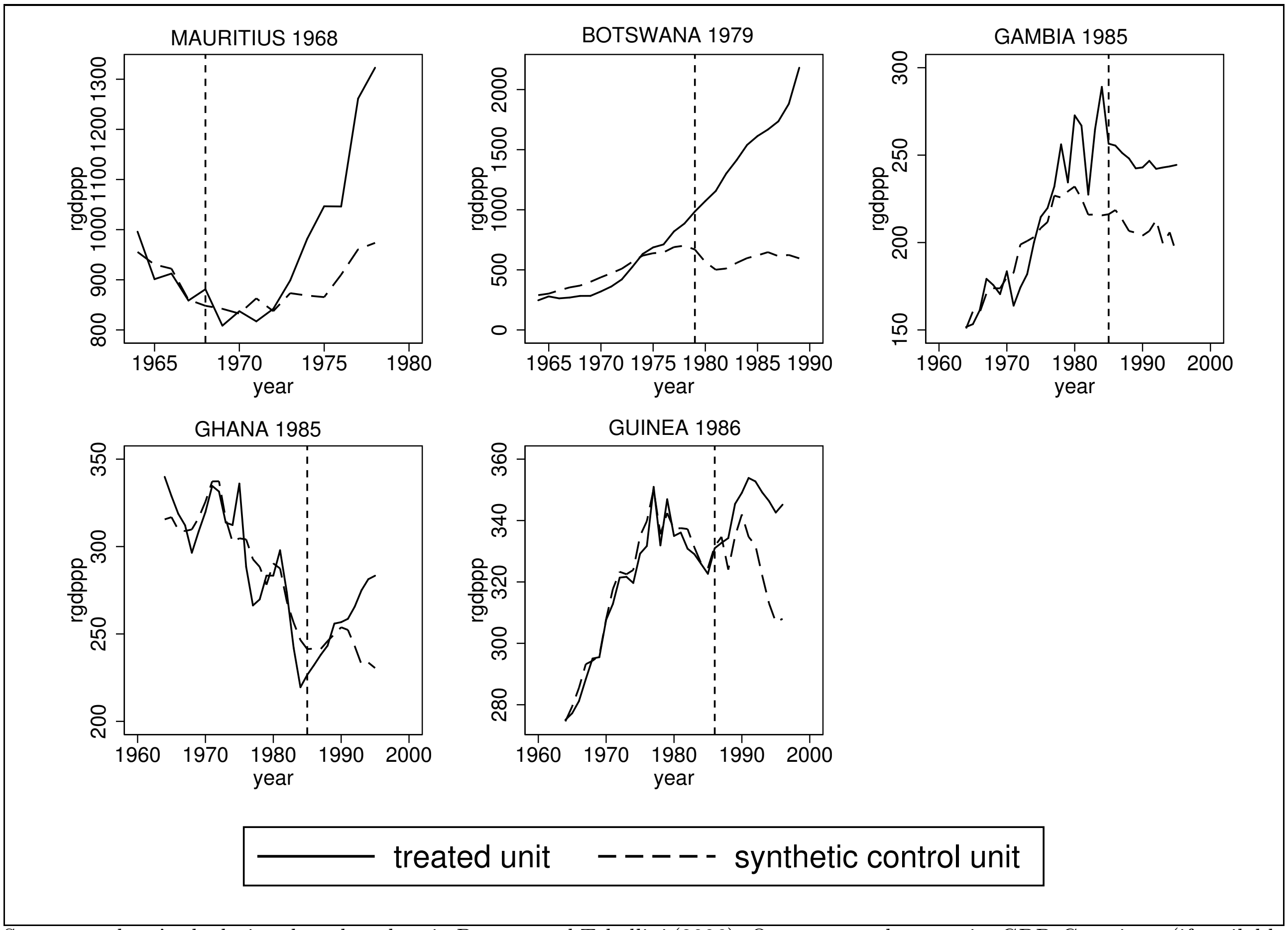
Source: authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Indonesia; synthetic control B for Singapore, South Korea, Philippines, and Nepal. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 2: GDP Trends, Treated Country vs. Synthetic Control — Latin America



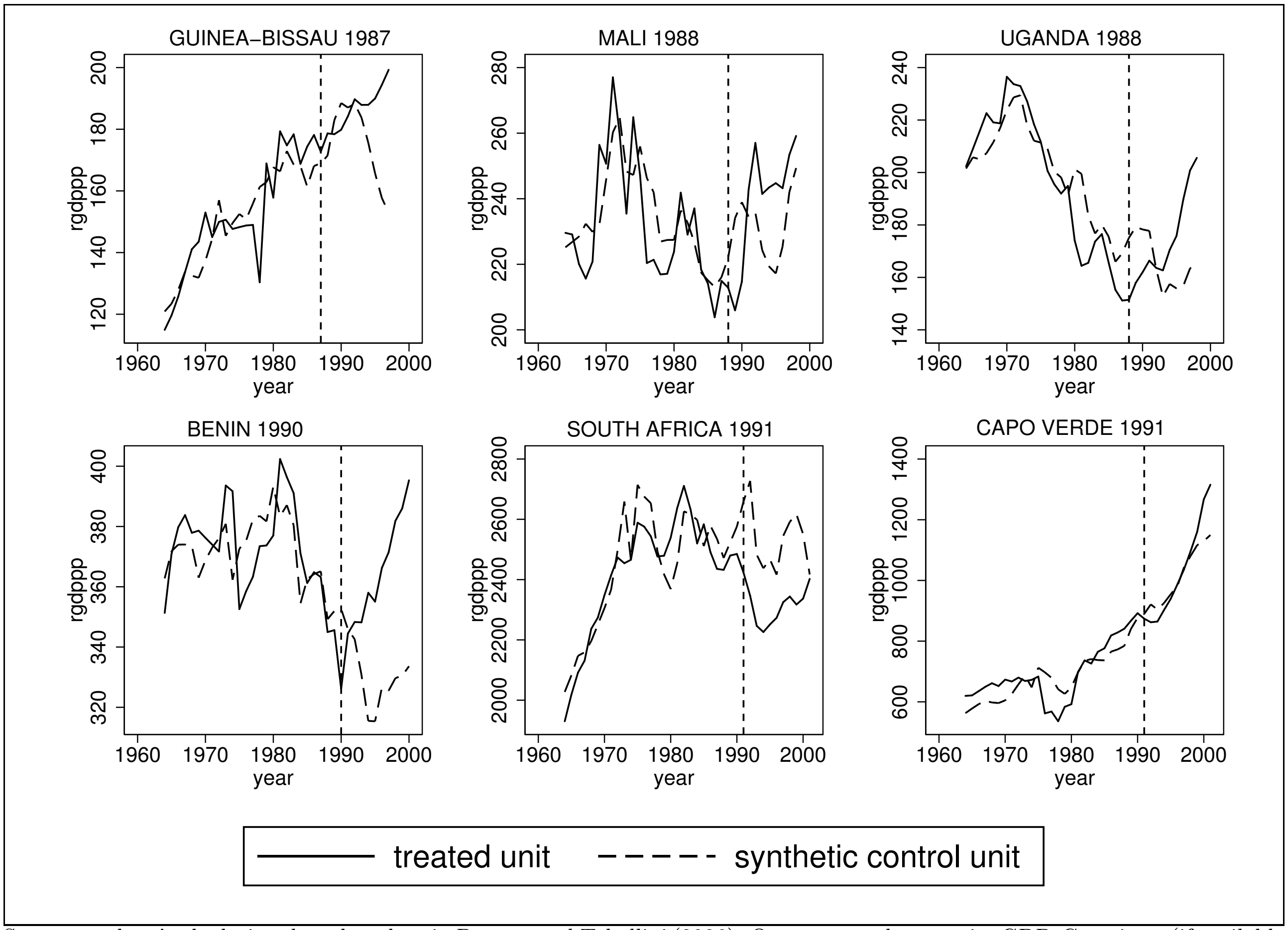
Source: authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Barbados, Costa Rica, and Mexico; synthetic control B for Chile and Colombia. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 3: GDP Trends, Treated Country vs. Synthetic Control — Africa Before 1987



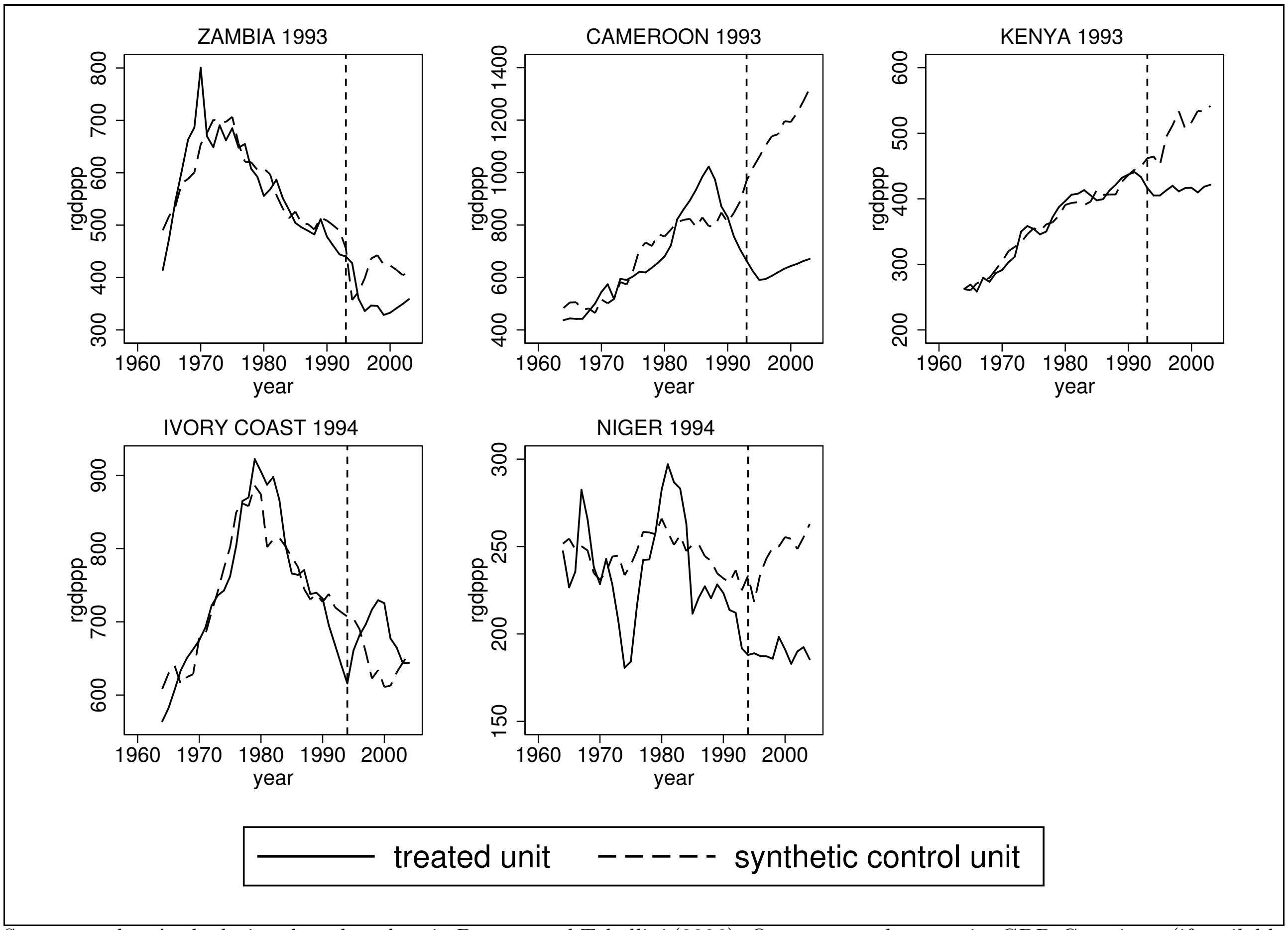
Source: authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Gambia, Ghana, and Guinea; synthetic control B for Mauritius and Botswana. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 4: GDP Trends, Treated Country vs. Synthetic Control — Africa Between 1987 and 1991



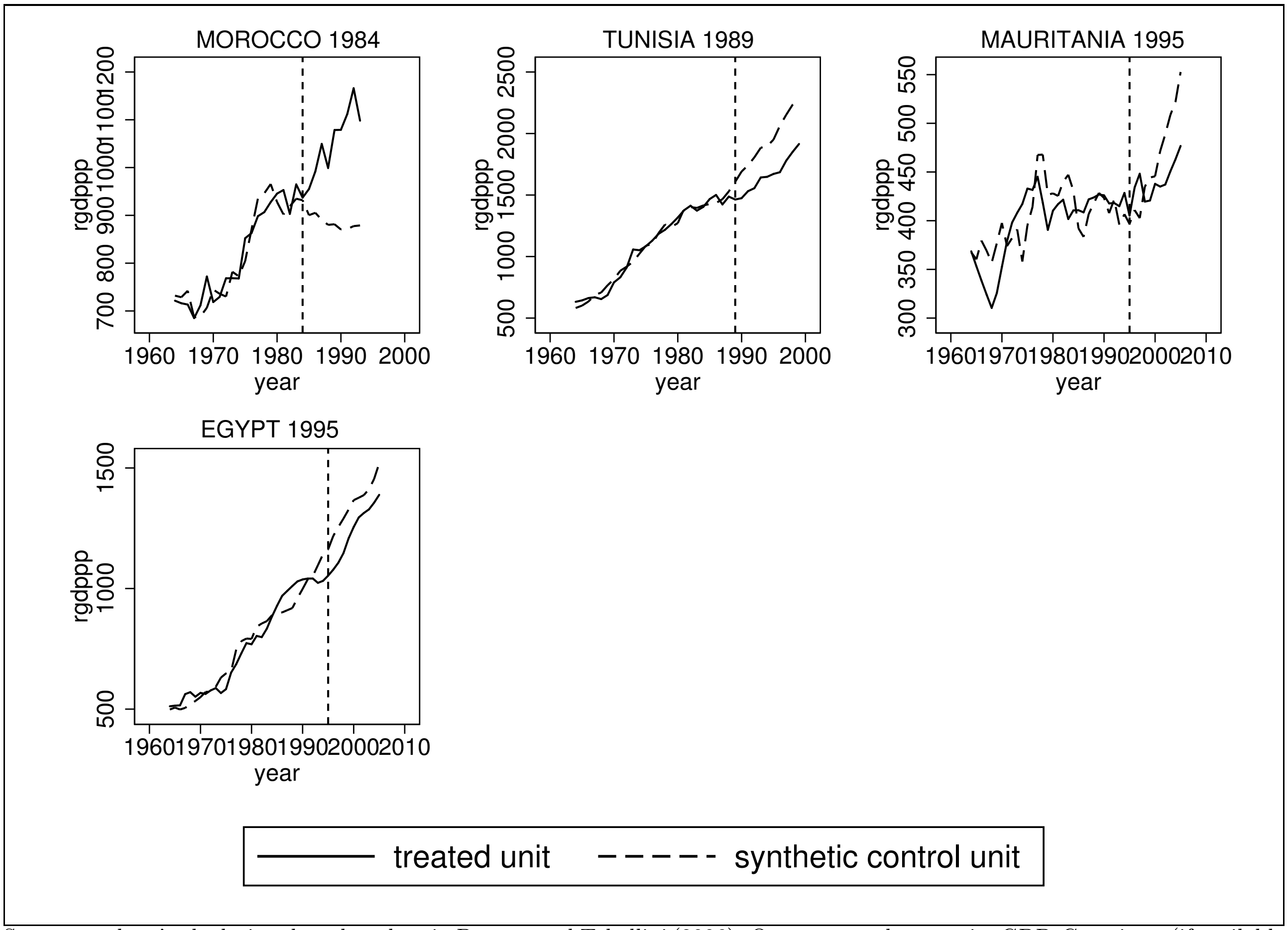
Source: authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Guinea-Bissau, Mali, Uganda, and Benin; synthetic control B for South Africa and Cape Verde. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 5: GDP Trends, Treated Country vs. Synthetic Control — Africa After 1991



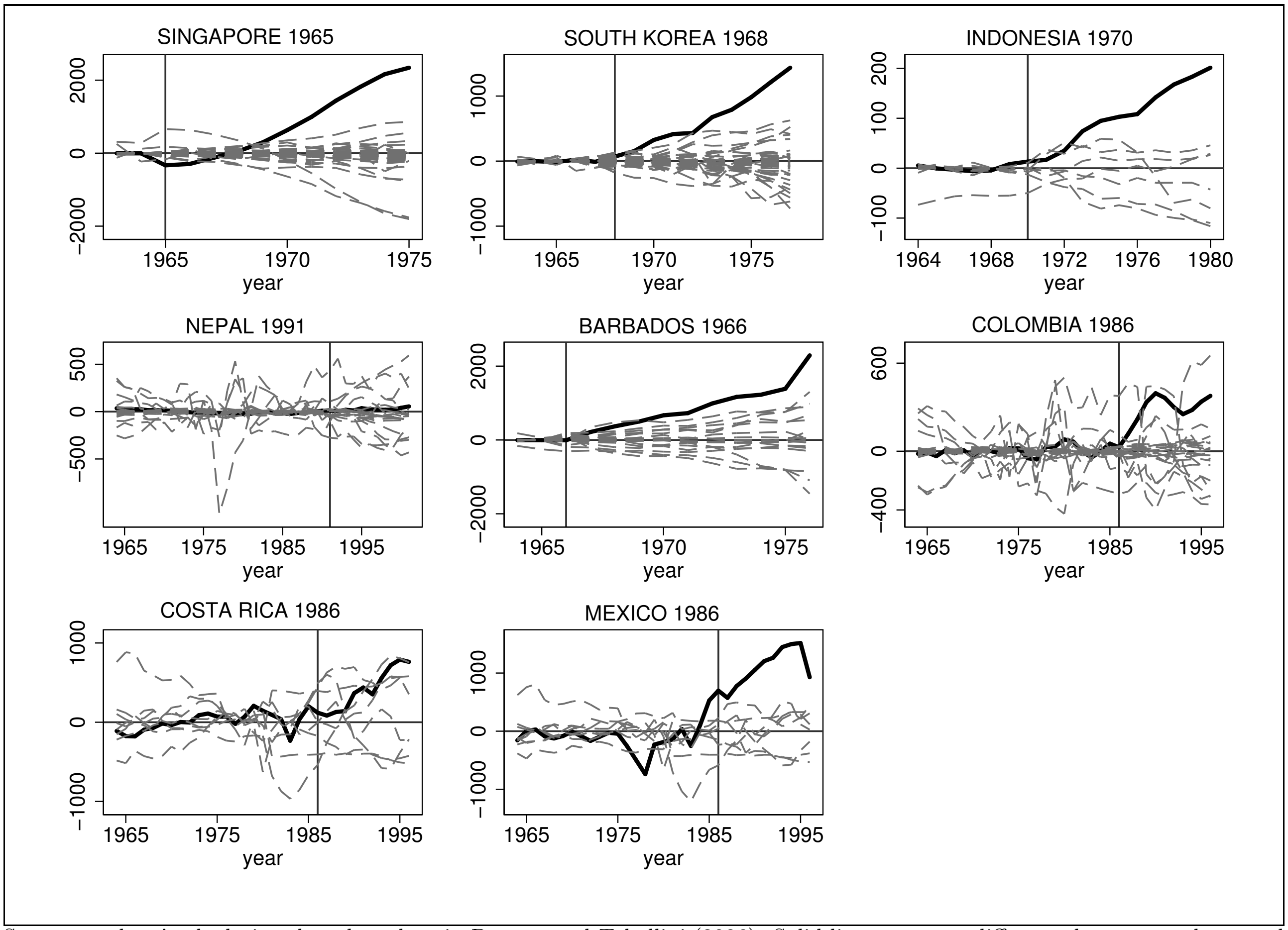
Source: authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Kenya and Niger; synthetic control B for Zambia, Cameroon, and Ivory Coast. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 6: GDP Trends, Treated Country vs. Synthetic Control — Middle East



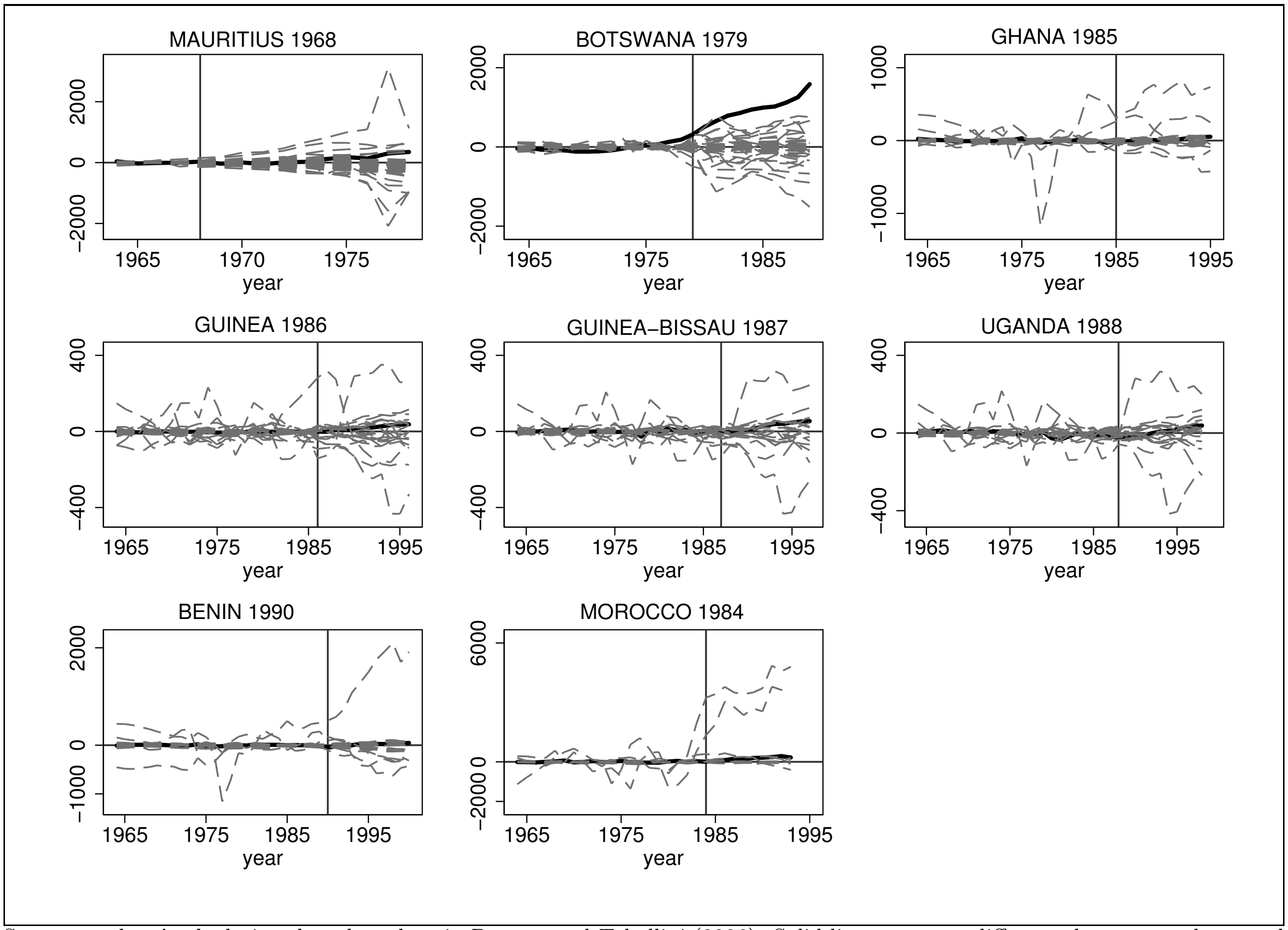
Source: authors' calculations based on data in Persson and Tabellini (2006). Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Morocco and Mauritania; synthetic control B for Tunisia and Egypt. See Tables 1 through 4 for the list of potential controls in each macro-region; see the Appendix for the list (and relative weights) of the countries actually included in each synthetic control.

Figure 7: Placebo Experiments — Asia & Latin America



Source: authors' calculations based on data in Persson and Tabellini (2006). Solid line: outcome difference between each treated country and its synthetic control. Dashed lines: outcome difference between each of the treated country's potential controls and their synthetic control in placebo experiments. Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Indonesia, Barbados, Costa Rica, and Mexico; synthetic control B for Singapore, South Korea, Nepal, and Colombia.

Figure 8: Placebo Experiments — Africa & Middle East



Source: authors' calculations based on data in Persson and Tabellini (2006). Solid line: outcome difference between each treated country and its synthetic control. Dashed lines: outcome difference between each of the treated country's potential controls and their synthetic control in placebo experiments. Outcome: real per capita GDP. Covariates (if available for at least one year before the treatment): secondary school enrollment, population growth, investment share, inflation, democracy, and pre-treatment real GDP per capita. Synthetic control A for Ghana, Guinea, Guinea-Bissau, Uganda, Benin, and Morocco; synthetic control B for Mauritius and Botswana.